LETTERS TO THE EDITOR.

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Carpenter on Eozoon.

THE scientific world is deeply indebted to Dr. Dallinger for his excellent new edition of Carpenter's invaluable work on the microscope, and among other things for his retaining unchanged the description of *Eozoon canadense*, as a monument of an important research up to a certain date.

Dr. Carpenter devoted much time to the study of Eozoon, and brought to bear on it his great experience of foraminiferal forms, and his wonderful powers of manipulating and unravelling difficult structures. After having spent years in studying microscopic slices of Eozoon and the limestones in which it occurs, I have ever felt new astonishment when I saw the manner in which, by various processes of slicing and etching, and by dexterous management of light, he could bring out the structure of specimens often very imperfect. Not long before Dr. Carpenter's death I had an opportunity to appreciate this in spending a few days with him in studying his more recently acquired specimens, some of them from my own collections, and discussing the new points which they exhibited, and which unhappily he did not live to publish. Some of these new facts, in so far as they related to specimens in our cabinet here, have since that time been noticed in my *résumé* of the question in the Memoirs of the Peter Redpath Museum, 1888; but I hope my friend Prof. Rupert Jones may yet be able to complete Dr. Carpenter's work.

Those who know Dr. Carpenter's powers of investigation will not be surprised that later observers, without his previous preparation and rare insight, and often with only few and imperfect specimens, should have failed to appreciate his results. One is rather surprised that some of them have ventured to state with so great confidence their own negative conclusions in a matter of so much difficulty, and requiring so much knowledge of organic structures in various states of mineralization. For myself, after working for fifty years at the microscopic examination of fossils and organic rocks, I feel more strongly than ever the uncertainties and liabilities to error which beset such inquiries.

As an illustration in the case of Eozoon: since the publication of my memoir of 1888, which I had intended to be final and exhaustive as to the main points, and in so far as I am concerned, I have had occasion to have prepared and to examine about 200 slices of Eozoon from new material: and while most of these have either failed to show the minute structures or have presented nothing new, a few have exhibited certain parts in altogether unexpected perfection, and have shown a prevalence of injection of the canal system by dolomite not previously suspected. Since that publication also, the discoveries of Mr. Matthew in the Laurentian of New Brunswick, and the further study of the singular Cambrian forms of the type of Cryptozoum, have opened up new fields of inquiry.

I think it proper to state, in reference to Dr. Dallinger's footnote on the recent paper of Mr. Gregory, that it must not be inferred from it that Mr. Gregory had access to my specimens from Madoc and Tudor, though he no doubt had excellent material from the collections of the Canadian Geological Survey. It might also be inferred from this note that I have regarded the Madoc and Tudor specimens as "Lower Laurentian." The fact is that I was originally induced in 1865, by the belief of Sir W. E. Logan at that time that these rocks were representatives in a less altered state of the middle part of the Laurentian, to spend some time at Madoc and its vicinity in searching for fossils, but discovered only worm-burrows, spicules, and fragments of Eozoon, which were noticed in the Journals of the Geological Society for 1866. (The more complete specimen from Tudor was found by Vennor in 1866.) On that occasion I satisfied myself fully that the beds are much older than the Cambro-Silurian strata resting on them unconformably; but I felt disposed to regard them as more probably of the age of some parts of the Huronian of Georgian Bay, which I had explored with a similar purpose under Logan's guidance in 1856. As my work was not official, and was palæontological rather than stratigraphical, it did not seem proper to express any dissent from what were at the time the probable conclusions of stratigraphical work; but I was quite prepared to assent to the new views afterwards adopted.

In conclusion, the new material bearing on Eozoon is accumulating so rapidly that I cannot hope to be able to master it in detail, but shall be glad to aid others who may have more time; but I hope to be able, in a work now in preparation, at least to present the facts up to date in a popular form.

J. WILLIAM DAWSON.

McGill College, Montreal, February 3.

The Samoan Hurricane.

REPLYING to the communication in NATURE of December 17, 1891 (p. 161), signed "H. F. B.," relative to my preliminary report on the Samoan hurricane of March 1889 (published in the Proceedings, U.S. Naval Institute, vol. xvii., No. 2, and in the *American Meteorological Journal*, July 1891), I would submit the following statement.

First of all, I wish to acknowledge Mr. Blanford's appreciation of the difficulties involved in the consideration of the subject, owing to the meagreness of the data; and at the same time to express my own appreciation of the fact that he himself has not had access even to such data as we have succeeded in collecting, but only to my necessarily brief discussion thereof, and the conclusions that I have drawn therefrom.

Mr. Blanford's explanation, as I understand it, is as follows: the vortex of the hurricane formed north or north-east (on the equatorial side) of Apia on the afternoon of the 15th, within a "region of disturbance" that had already caused stormy weather throughout the Samoan Islands and a decided barometric depression at Apia. The first effect of this formation was (by adding slightly to the normal evening rise of the barometer) to cause a decided rise of pressure, which, however, decreased again as the vortex slowly approached the harbour, thus causing the second minimum (the afternoon of the 16th), the duration of the storm being explained by the usual slow motion of the newly formed hurricane.

To the above explanation it is necessary to make a correction, I think, owing to the fact that the shifts of wind at the time of and immediately after the first minimum show that the centre of the disturbance then passed to the westward of Apia, and as the wind thereafter remained from north to north-east, the centre (or vortex) evidently remained to the southward and westward. This fact, however, merely introduces a change in the position where the new vortex formed, according to the theory under discussion.

Revising Mr. Blanford's explanation, then, in the light of this correction, it appears that the track of the *depression* is about as I have drawn it, but that a *vortex* formed slightly to the *southward* and *westward* of Apia, thus causing a slight rise of pressure at first, succeeded by a second fall, and the slow motion of this newly formed vortex caused the duration of the northerly gale.

Now, I must here take exception to one of Mr. Blanford's statements, which is as follows (referring to the theories given in my paper): "None of these explanations seem to take account of the circumstances that attend the formation of tropical cyclones, which, as we have elsewhere pointed out, differ in many respects from the storms of the temperate zone." A reference to my paper will show, I think, that I took into consideration the special peculiarity to which Mr. Blanford calls attention, and went so far as to insert a plate in order to illustrate two types of storms—namely, (1) the characteristic tropical hurricane type (where there is a decided vortex, or "centre of aspiration"); and (2) the type where there is a comparatively wide central region surrounded by an annular space where there are steep barometric gradients and correspondingly high wind velocities, but without any decided vortex, properly so called. I said also that "it will be seen that the indications are that the Samoan hurricane (on the 15th and 16th, at least) was of the second type, although during the 17th and 18th it doubtless became more like the first." In a word, I said (both explicitly and by means of the varying strength of the track drawn on the chart) that the depression passed Apia on the afternoon of the 15th, recurved (increasing in intensity and delaying whilst recurving, each of which is to be expected), and then moved off to the southward and eastward. I do not intend to convey the impression that I made any definite statement as to just when or where the vortex formed, nor am I wholly prepared to hazard such a statement even at

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this late date. I can still only repeat here what I said before, as follows:—

"My idea is, briefly, that the first depression occurred as the storm passed on its westward track, followed by the usual shift of wind to the northward. Along this branch of its trajectory its severity was probably not quite so great as it was later, and the force of its southerly winds was masked by the mountains on the island of Upolu; possibly careful observations of the rapidity of motion and the character of the clouds, or of the state of the sea off the harbour, might have indicated a severe storm, but this does not appear from the evidence at hand, though well worth considering. During its recurve the hurricane probably increased in intensity, the barometric depression at the centre deepening, and thus causing the second depression observed at Apia, which was slightly deeper than the first, although the centre itself was really at a greater distance than on the previous day."

Unfortunately, as stated above, we have no very definite data regarding the severity of the storm before it reached Apia, although I must say that a barometric depression of 0·76 inch below the normal in the tropics is very suggestive of the presence of a fully developed vortex, and it seems more than likely that if the harbour had not been well sheltered to the southward the southerly gales would have been quite as severe as those from the north. The only early data that we have, other than the reports from Apia, are contained in the following very brief report from the American schooner *Equator*, Captain Reid:—

> "March 14 (Samoan date), lat. 12° 00′ S., long. 170° 50′ W., wind S., S.W., W., N.W.; thick, squally; hove to.
>
> "March 15 (Samoan date), lat. 13° 00′ S., long. 170° 40′ W., wind N.W.; fierce gale; squalls; heavy sea."

This, although brief, seems to indicate that it was a fully developed hurricane that was approaching Apia, and I am inclined to the opinion that such was actually the case.

Mr. Blanford will surely admit that it is one recognized peculiarity of tropical hurricanes to reduce their speed of translation whilst they are recurving, and he will thus admit also that such recurvature accounts, in the present case, for the duration of the northerly gale at Apia. So far as the sharpness of recurvature is considered, I would say that the report of the *Equator* is sufficiently vague to allow us to draw a curve with a somewhat less sharp recurvature, and I should myself have done so but for the single fact that a hurricane approaching from a more northerly direction must have sent a northerly or north-easterly swell into the harbour that would have been felt to at least such an extent as to be noted in the log of one or more of the vessels there, but I have looked in vain for any such remark, until after the shift of wind on the afternoon of the 15th.

In conclusion, I take pleasure in expressing my obligation to Mr. Blanford for his interesting and able discussion of the subject, which, even with all the data at hand, has still many perplexing features. The principal object of this preliminary publication was to elicit comment and discussion, and it will be very gratifying if other authorities will give us the benefit of their experience and suggestions.

EVERETT HAYDEN.

Hydrographic Office, Washington, D.C.,
January 4.

THE distinction to which allusion was made in the passage quoted by Mr. Hayden is not, as he seems to have understood, that between tropical and extra-tropical storms when fully formed, but between the circumstances of their respective formation. These are that, in the former, the cyclonic circulation of the winds is preceded by much irregular action, which sometimes extends over a considerable area. Within this area there are local squalls and shifts of wind, with heavy rainfall, but the action is not for some time definitely concentrated and cyclonic. This preliminary stage does not appear to obtain in the storms of the temperate zone, where the deviating effect of the earth's rotation is so much greater than in low latitudes, and indeed, if we accept the views of Werner Siemens and Prof. Hann, the cyclonic circulation is the cause and forerunner of the storm. I cannot think it probable that a vortex, once fully formed, and travelling towards higher latitudes, should recurve so sharply as to produce a fall of the barometer on two successive days (with a rise in the interval) at the same place, simply by twice passing in its vicinity. To effect this, the recurvature must, as I apprehend, describe an angle considerably less than a right angle, and of such I know of no example among tropical cyclones. At the same time, my own view was put forward merely as a suggestion, and in no dogmatic spirit.

H. F. B.

Phoronomy.

ABOUT thirty-five years ago, I had a conversation with the late Dr. Donaldson, a well-known Greek scholar of the time, in which we discussed the appropriateness of the use of the word kinematics, in the sense in which it was then, and is now, employed by writers on mathematical science. Dr. Donaldson's opinion was that it is not the best word which can be employed to represent the science of pure motion, without regard to causation. He said that the word κινέω involved the idea of the cause of motion, and therefore that it ought not to be used when the idea of causation is to be completely set aside. He further gave it as his opinion that the word φορέω is more nearly expressive of the idea of mere going, without any reference to the cause of motion, and therefore that the proper word would be phoronomy, or phoretics.

I was so much impressed by this conversation that, for many years, I headed with the word phoronomy the papers of questions on the subject of pure motion which I was in the habit of preparing for College lectures and private pupils.

I have recently consulted a very eminent Greek scholar, and his opinion is that, on the whole, the word phoronomy is more distinctly expressive of the science of pure motion than the word kinematics. He agreed with Dr. Donaldson that the word kinematics suggests some idea of causation, whereas no such idea is suggested by the word phoronomy.

As a matter of history, the word *cinématique* was introduced by Ampère to represent the purely geometrical science of motion in the abstract, and was anglicized into kinematics by, I think, the late Dr. Whewell.

Sir W. Thomson and Prof. Tait, in the preface to their "Natural Philosophy," adopt the suggestion of Ampère, and employ the word in the same sense. They also employ the word *dynamics* in its true sense as the science which treats of the action of *force*, whether it maintains relative rest, or produces acceleration of relative motion. They further state that these two corresponding divisions of dynamics are conveniently entitled *statics* and *kinetics*.

Here, then, we have two words, kinematics and kinetics, both derived from the same root-word, employed to represent two entirely different sets of ideas; and there is not the same broad line of demarcation between the words themselves as there is between the sets of ideas which they are intended to connote.

Hence it appears to me that the word phoronomy, the law of going, is the most suitable, as it is the most expressive word, to represent the science of pure motion in the abstract.

At the time of my conversation with Dr. Donaldson, we were neither of us aware that the word had been already invented and employed. Some years after, I found that a treatise was published at Amsterdam in 1716, entitled "Phoronomia, sive de viribus et motibus corporum. Autore Jacobo Hermanno, Basil." Hermann, however, uses the word in much the same sense as we now use the word dynamics.

The point has been recalled to my mind by the discovery that the word has been employed in Germany in the sense in which Dr. Donaldson advocated that it should be used.

In the treatise entitled "Allgemeine Mechanik der Punkte und Starren Systeme," by E. Budde, published in 1890 at Berlin, the word phoronomy is adopted, and the author gives his reasons in the following words:—

"Man kann eine Ortsveränderung zunächst rein geometrisch, ohne alle Rücksicht auf ihre Ursachen, betrachten, und das soll in den nächsten Capiteln geschehen. Die Disciplin, welche sich mit dieser Betrachtung befasst, heisst *Phoronomie* oder *Kinematik*. Der Name Kinematic ist seit Resal der gebräuchlichere gewesen; neuerdings aber wird von Reulaux und seinen Schülern die Morphologie der Verknüpfung von Maschinentheilen als 'Kinematik' bezeichnet. Wir wählen deshalb hier den Namen Phoronomie."

With reference to this statement of Budde's, I observe that Ampère, in the "Essaie sur la Philosophie des Sciences," particularly mentions trains of machinery, such, for instance as the works of a watch, as coming under the heading kinematics.

I also find that Grassmann, in the "Ausdehnungslehre von

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